XXVII. *An Experimental Examination of the Quantity and Proportion of Mechanic Power necessary to be employed in giving different Degrees of Velocity to Heavy Bodies from a State of Rest. By Mr.* John Smeaton, *F. R. S.*

R. April 25, 1776.

ABOUT the year 1686 Sir ISAAC NEWTON first published his *Principia*, and, conformably to the language of mathematicians of those times defined, that "the quantity of motion is the "measure of the same, arising from the velocity and "quantity of matter conjointly." Very soon after this publication, the truth or propriety of this definition was disputed by certain philosophers, who contended, that the measure of the quantity of motion should be estimated by taking the quantity of matter and the square of the velocity conjointly. There is nothing more certain, than that from equal impelling powers, acting for equal intervals of time, equal increases of velocity are acquired by given bodies, when unresisted by a medium; thus gravity causes a body, in obeying its impulse during one second of time, to acquire a velocity which would carry it uniformly forward, without any additional impulse, at the rate of 32 ft. 2 in. *per* second; and if gravity is suffered to act upon it for two seconds, it

will have, in that time, acquired a velocity that would carry it, at an uniform rate, juſt double of the former; that is, at the rate of 64 ft. 4 in. *per* ſecond. Now, if in conſequence of this equal increaſe of velocity, in an equal increaſe of time, by the continuance of the ſame impelling power, we define that to be a double quantity of motion, which is generated in a given quantity of matter, by the action of the ſame impelling power for a double time; this will be co-incident with Sir ISAAC NEWTON's definition abovementioned; whereas, in trying experiments upon the total effects of bodies in motion, it appears, that when a body is put in motion, by whatever cauſe, the impreſſion it will make upon an uniformly reſiſting medium, or upon uniformly yielding ſubſtances, will be as the maſs of matter of the moving body, multiplied by the ſquare of its velocity: the queſtion, therefore, properly is, whether thoſe terms, the *quantity of motion*, the *momenta* of bodies in motion, or *forces* of bodies in motion, which have generally been eſteemed ſynonymous, are with the moſt propriety of language to be eſteemed equal, double, or triple, when they have been generated by an equable impulſe, acting for an equal, double, or triple time; or that it ſhould be meaſured by the effects being equal, double, or triple, in overcoming reſiſtances before a body in motion can be ſtopped? For, according as thoſe terms are underſtood in this or that way, it will neceſſarily follow, that the *momenta* of equal bodies will be as the velocities, or as the ſquares of the velocities reſpectively; it being certain,

 that,

that, whichever we take for the proper definition of the term quantity of motion, by paying a proper regard to the collateral circumftances that attend the application of it, the fame conclufion, in point of computation, will refult. I fhould not, therefore, have thought it worth while to trouble the Society upon this fubject, had I not found, that not only myfelf and other practical artifts, but alfo fome of the moft approved writers, had been liable to fall into errors, in applying thefe doctrines to practical mechanics, by fometimes forgetting or neglecting the due regard which ought to be had to thefe collateral circumftances. Some of thefe errors are not only very confiderable in themfelves, but alfo of great confequence to the public, as they tend greatly to miflead the practical artift in works that occur daily, and which often require very great fums of money in their execution. I fhall mention the following inftances.

DESAGULIERS, in his fecond volume of Experimental Philofophy, treating upon the queftion concerning the forces of bodies in motion, after taking much pains to fhew that the difpute, which had then fubfifted fifty years, was a difpute about the meaning of words; and that the fame conclufion will be brought out, when things are rightly underftood, either upon the old or new opinion, as he diftinguifhes them; among other things, tells us, that the old and new opinion may be eafily reconciled in this inftance: that the wheel of an underfhot water-mill is capable of doing quadruple work when the velocity of the water is doubled, inftead of

double work only; " becaufe (the adjutage being the " fame), fays he, we find, that as the water's velocity is " double, there are twice the number of particles of " water that iffue out, and therefore the ladle-board is " ftruck by twice the matter, which matter moving with " twice the velocity that it had in the firft cafe, the " whole effect muft be quadruple, though the inftan- " taneous ftroke of each particle is increafed only in a " fimple proportion of the velocity." See vol. II. Annotations on lecture 6th, p. 92.

Again, in the fame volume, lecture 12th, p. 424. referring to what went before, he tells us, " The know- " ledge of the foregoing particulars is abfolutely necef- " fary for fetting an underfhot wheel to work; but the " advantage to be reaped from it would be ftill guefs- " work, and we fhould be ftill at a lofs to find out the " utmoft it can perform, if we had not an ingenious " propofition of that excellent mechanic M. PARENT, of " the Royal Academy of Sciences, who has given us a " *maximum* in this cafe, by fhewing, that an underfhot " wheel can do the moft work, when its velocity is equal " to the third part of the velocity of the water that " drives it, &c. becaufe then two-thirds of the water is " employed in driving the wheel with a force propor- " tionable to the fquare of its velocity. If we mul- " tiply the furface of the adjutage or opening by the " height of the water, we fhall have the column of wa- " ter that moves the wheel. The wheel thus moved will " fuftain on the oppofite fide only four-ninths of that

 " weight,

"weight, which will keep it in equilibrio; but what it "can move with the velocity it goes with, will be but "one-third of that weight of equilibrium; that is, $\frac{4}{27}$ths "of the weight of the firft column, &c.—This is the "utmoft that can be expected."

The fame conclufion is likewife adopted by MACLAURIN, in art. 907. p. 728. of his Fluxions, where, giving the fluxionary deduction of M. PARENT's propofition, he fays, "that if A reprefents the weight which would "balance the force of the ftream, when its velocity is "a; and U reprefents the velocity of the part of the en- "gine, which it ftrikes when the motion of the machine "is uniform, &c.—the machine will have the greateft "effect when U is equal to $\frac{a}{3}$; that is, if the weight that "is raifed by the engine be lefs than the weight which "would balance the power, in the proportion of 4 to 9, "and the *momentum* of the weight is $\frac{4Aa}{27}$."

Finding that thefe conclufions were far from the truth; and feeing, from many other circumftances, that the practical theory of making water and wind-mills was but very imperfectly delivered by any author I had then an opportunity of confulting [(a)]; in the year 1751 I began a courfe

(a) BELIDOR, *Architecture Hydraulique*, greatly prefers the application of water to an underfhot mill, inftead of an overfhot; and attempts to demonftrate, that water applied underfhot will do fix times more execution than the fame applied overfhot. See vol I. p. 286. While DESAGULIERS, endeavouring to invalidate what had been advanced by BELIDOR, and greatly preferring an over-fhot

a courſe of experiments upon this ſubject. Theſe experiments, with the concluſions drawn from them, have already been communicated to this Society, who printed them in vol. LI. of their Tranſactions for the year 1759, and for this communication I had the honour of receiving the annual medal of Sir GODFREY COPLEY, from the hands of our very worthy Preſident the late Earl of MACCLESFIELD. Thoſe experiments and concluſions ſtand uncontroverted, ſo far as I know, to this day; and having ſince that time been concerned in directing the conſtruction of a great number of mills, which were all executed upon the principles deduced from them, I have by that means had many opportunities of comparing the effects actually produced with the effects which might be expected from the calculation; and the agreement, I have always found between theſe two, appears to me fully to eſtabliſh the truth of the principles upon

ſhot to an underſhot, ſays, Annotat. on lecture 12. vol. II. p. 532. that from his own experience, "a well-made overſhot mill ground as much corn in the "ſame time with ten times leſs water;" ſo that betwixt BELIDOR and DESAGULIERS here is a difference of no leſs than 60 to 1.

Again, BELIDOR, vol. II. p. 72. ſays, that the center of gravity of each ſail of a wind-mill ſhould travel in its own circle with one-third of the velocity of the wind; ſo that, taking the diſtance of this center of gravity from the center of motion at 20 feet, as he ſtates it p. 38. art. 849. the circumference will be exceeding 126 feet Engliſh meaſure: a wind, therefore, to make the mill go twenty turns *per* minute, which they frequently do with a freſh wind and all their cloth ſpread, would require the wind to move above eighty miles an hour; a velocity perhaps hardly equalled in the greateſt ſtorms we experience in this climate.

which

which they were conſtructed, when applied to great works, as well as upon a ſmaller ſcale in models.

Reſpecting the explanatory deduction of DESAGULIERS in the firſt example abovementioned, which, indeed, I have found to be the commonly received doctrine among theoretical mechanics, it is ſhewn, Phil. Tranſ. vol. LI. p. 120, 121, and 123. part I. maxim 4. that, where the velocity of water is double, the adjutage or aperture of the ſluice remaining the ſame, the effect is eight times; that is, not as the ſquare but as the cube of the velocity; and the ſame is inveſtigated concerning the power of the wind ariſing from difference of velocity, p. 156. being part 3. maxim 4.

The concluſion in the ſecond example abovementioned, adopted both by DESAGULIERS and MACLAURIN, is not leſs wide of the truth than the foregoing; for if that concluſion were true, only $\frac{4}{27}$ths of the water expended could be raiſed back again to the height of the reſervoir from which it had deſcended, excluſively of all kinds of friction, &c. which would make the actual quantity raiſed back again ſtill leſs; that is, leſs than one-ſeventh of the whole; whereas it appears, from table I. p. 115. of the ſaid volume, that in ſome of the experiments there related, even upon the ſmall ſcale on which they were tried, the work done was equivalent to the raiſing back again about one quarter of the water expended; and in large works the effect is ſtill greater, approaching towards half, which ſeems to be the limit for the underſhot mills, as the whole would be the limit for the

the overſhot mills, if it were poſſible to ſet aſide all friction, reſiſtance from the air, &c. ſee p. 130.

The velocity alſo of the wheel, which, according to M. PARENT's determination, adopted by DESAGULIERS and MACLAURIN, ought to be no more than one-third of that of the water, varies at the *maximum* in the above-mentioned experiments of table 1. between one third and one half; but in all the caſes there related, in which the moſt work is performed in proportion to the water expended, and which approach the neareſt to the circumſtances of great works, when properly executed, the *maximum* lies much nearer to one half than one third; one half ſeeming to be the true *maximum*, if nothing were loſt by the reſiſtance of the air, the ſcattering of the water carried up by the wheel, and thrown off by the centrifugal force, &c. all which tend to diminiſh the effect more, at what would be the *maximum* if theſe did not take place, than they do when the motion is a little ſlower.

Finding theſe matters, as well as others, to come out in the experiments, ſo very different from the opinions and calculations of authors of the firſt reputation, who, reaſoning according to the Newtonian definition, muſt have been led into theſe errors from a want of attending to the proper collateral circumſtances; I thought it very material, eſpecially for the practical artiſt, that he ſhould make uſe of a kind of reaſoning in which he ſhould not be ſo liable to miſtakes; in order, therefore, to make this matter perfectly clear to myſelf, and poſſibly ſo to others,

I reſolved

I refolved to try a fet of experiments from whence it might be inferred, what proportion or quantity of mechanical power is expended in giving the fame body different degrees of velocity. This fcheme was put in execution in the year 1759, and the experiments were then fhewn to feveral friends, particularly my very worthy and ingenious friend Mr. WILLIAM RUSSELL.

In my experimental inquiry concerning the powers of water and wind before referred to, I have, p. 105. part 1. defined what I meant by power, as applied to practical mechanics, that is, what I now call mechanical power; which, in terms fynonymous to thofe there ufed, may be faid to be meafured by multiplying the weight of the body into the perpendicular height from which it can defcend; thus the fame weight, defcending from a double height, is capable of producing a double mechanical effect, and is therefore a double mechanical power. A double weight defcending from the fame height is alfo a double power, becaufe it likewife is capable of producing a double effect; and a given body, defcending through a given perpendicular height, is the fame power as a double body defcending through half that perpendicular; for, by the intervention of proper levers, they will counter-balance one another, conformably to the known laws of mechanics, which have never been controverted. It muft, however, be always underftood, that the defcending body, when acting as a meafure of power, is fuppofed to defcend flowly, like the weight of a clock or a jack; for, if quickly defcending,

it is fenfibly compounded with another law, *viz.* the law of acceleration by gravity.

DESCRIPTION OF THE MACHINE.

AB is the bafe of the machine placed upon a table.

AC is a pillar or ftandard.

CD is an arm, upon the extremity of which is fixed a plate *fg*, which is here feen edge-ways, through which is a fmall hole for receiving a fmall fteel pivot *e*, fixed in the top of the upright axis *e*B; the lower end of this axis finifhes in a conical fteel point, which refts upon a fmall cup of hard fteel polifhed at B.

HI is a cylinder of white fir, which paffes through a perforation in the axis, and therein fixes; and, upon the two arms formed thereby, are capable of fliding

KL two cylindric weights of lead of equal fize, which are capable of being fixed upon any part of the cylindric arms, from the axis to their extremities, by means of two thin wedges of wood. The two weights, therefore, being at equal diftances from the center, and the axis perpendicular, the whole will be balanced upon the point at B, and moveable thereupon by an impelling power, with very little friction.

Upon the upper part of the axis are formed MN, two cylindrical barrels, whereof M is double the diameter of N; they have a little pin ftuck into one fide of each at *o*, *p*.

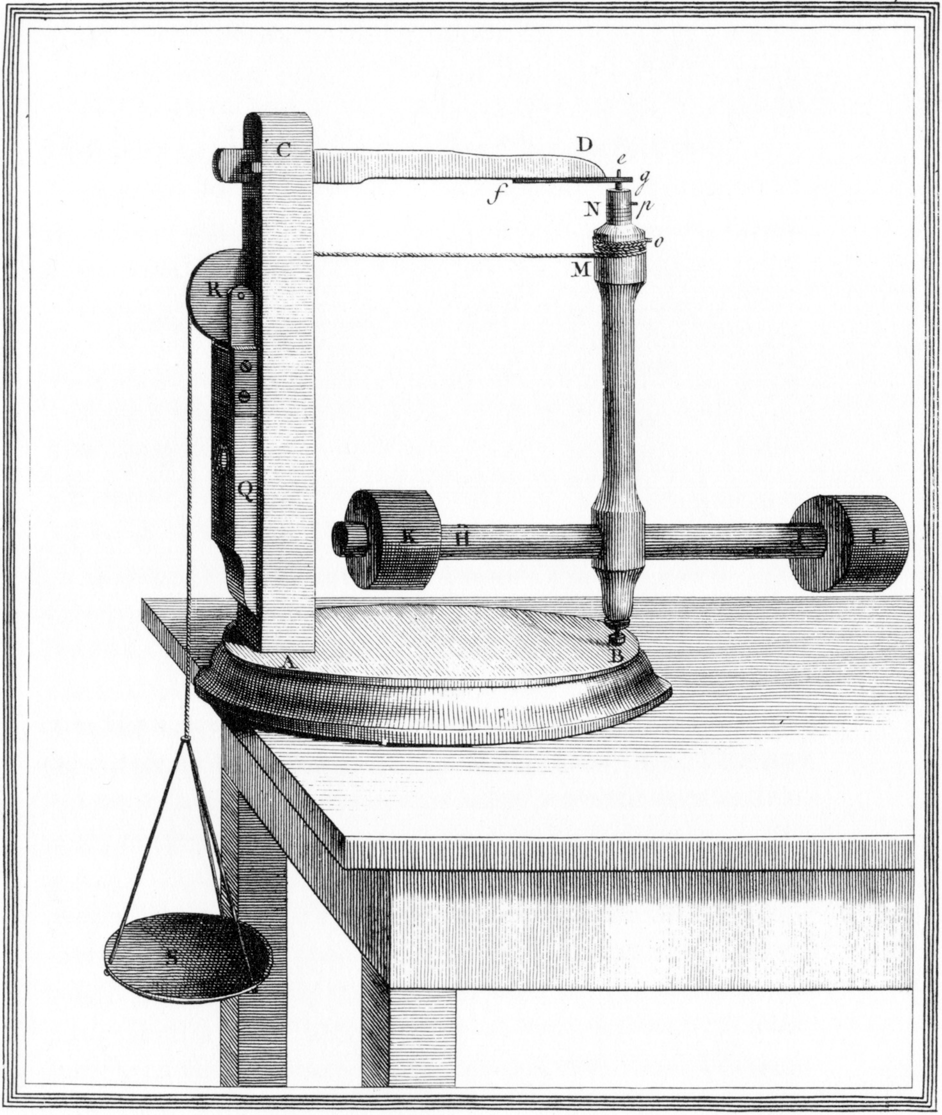
Q is a piece capable of fliding higher or lower, as occafion requires; and carries

R a light pulley of about three inches diameter, hung upon a ſteel axis, and moveable upon two ſmall pivots. The plane of the pulley, however, is not directed to the middle of the upright axis, but a little on one ſide, ſo as to point (at a mean) between the ſurface of the bigger barrel and the leſs.

S is a light ſcale for receiving weights, and hangs by a ſmall twine, cord, or line, that paſſes the pulley, and terminates either upon the bigger barrel or the leſs, as may be required; the ſliding-piece Q being moved higher or lower for each, that the line, in paſſing from the pulley to the barrel, may be nearly horizontal. The end of the line, that is furtheſt from the ſcale, is terminated by a ſmall loop, which hangs on upon the pin *o*, or the pin *p*, according as the bigger or the leſſer barrel is to be uſed.

Now, having wound up a certain number of turns of the line upon the barrel, and having placed a weight in the ſcale S, it is obvious, that it will cauſe the axis to turn round, and give motion to its arms, and to the weights of lead placed thereon, which are the heavy bodies to be put in motion by the impulſe of the weight in the ſcale; and when the line is wound off to the pin, the loop ſlips off, and the ſcale then falling down, the weight will ceaſe to accelerate the motion of the heavy bodies, and leave them revolving, equably forward, with the velocity they have acquired, except ſo far as it muſt be gradually leſſened by the friction of the machine and reſiſtance of the air, which being ſmall, the bodies will revolve ſometime before their velocity is apparently diminiſhed.

MEASURES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MEASURES OF SOME PARTS OF THE MACHINE.

	inches.
Diameter of the cylinders of lead, or the heavy bodies,	2,57
Length of ditto,	1,56
Diameter of the hole therein,	,72

Weight of each cylinder 3 lbs. Avoirdupois.

Greater diſtance of the middle of each body from the center of the axis,	8,25
The ſmaller diſtance of ditto,	3,92
10 turns of the ſmaller barrel raiſes the ſcale, 5 ditto of the bigger ditto,	25,25

When the bodies are at the ſmaller diſtance above ſpecified from the axis of rotation, they are then in effect at half the greater diſtance from that axis; for, ſince the axis itſelf, and the cylindric arms of wood, keep an unvaried diſtance from the center of rotation, the bodies themſelves muſt be moved nearer than half their former diſtance, in order that, compounded with the unvariable parts, they may be virtually at the half diſtance. In order to find this half diſtance nearly, I put in an arm of the ſame wood, that only went through the axis, without extending in the oppoſite direction; one of the bodies being put upon the end of this arm, at the diſtance of 8,25 inches, the whole machine was inclined till the body and arm became a kind of pendulum, and vibrated

at the rate of 92 times *per* minute; and as a pendulum of the half length vibrates quicker in the proportion of $\sqrt{1}$ to $\sqrt{2}$; that is, in the proportion of 92 to 130 nearly; therefore, keeping the ſame inclination of the machine, the weight was moved upon the arm till it made 130 vibrations *per* minute; which was found to be, when it was at 3,92 inches diſtance from the center as above ſtated, which is about $\frac{2}{10}$ths of an inch nearer than the half diſtance. The double arm was then put in, and marked accordingly, and the bodies being mounted thereon, the whole was adjuſted ready for uſe; and with it were tried the following experiments, each of which was repeated ſo many times as to be fully ſatisfactory.

TABLE

TABLE OF EXPERIMENTS.

N°	Ounces Avoirdupois in the Scale.	Barrel used, M the bigger, N the smaller.	The Arms, W the whole, H the Half-length.	Number of Turns of the Line wound on the Barrel.	Time of the Descent of the Weight in the Scale.	Time in making 20 Revolutions with equable Motion.
1	8	M	W	5	$14\frac{1}{4}''$	$29''$
2	8	N	W	10	$28\frac{1}{4}$	$29\frac{1}{4}$
3	8	N	W	$2\frac{1}{2}$	$14\frac{1}{4}$	$58\frac{1}{2}$
4	32	M	W	5	7	14
5	32	N	W	10	14	$14\frac{3}{4}$
6	32	N	W	$2\frac{1}{2}$	7	$28\frac{3}{4}$
7	8	M	H	5	7	$14\frac{3}{4}$
8	8	N	H	10	14	15
9	8	N	H	$2\frac{1}{2}$	7	$30\frac{1}{4}$
1	2	3	4	5	6	7

The $58''\frac{1}{2}$ in number 3, column 7, was determined in fact from $29''\frac{1}{4}$, being the time of making 10 equable revolutions, after the weight was dropped off, in order to prevent the sensible retardation that might take place, and affect the observation, if continued for 20 revolutions made so slowly.

FURTHER

FURTHER DEFINITIONS.

I have already defined what I mean by mechanic power; but, before I proceed further, it will be neceſſary alſo to define the following terms:

Impulſe or Impulſion,
Impulſive Force or Power,
Impelling Force or Power,

By all which, I underſtand the uniform endeavour that one body exerts upon another, in order to make it move; and that, whether it produces or generates motion by this endeavour or not; and the quantity of this impelling power may be meaſured either by its being a weight of itſelf, or by being counter-balanced by a weight. It may alſo act either immediately upon the body to be moved, ſo that if motion is the conſequence, they move with the ſame velocity; and that, either by a ſimple contact, or by being drawn as by a cord, or puſhed as by a ſtaff: or it may act by the intervention of a lever or other mechanic inſtrument, in which the velocity of the body to be moved may be very different from the velocity of the impelling power or mover; but in comparing them, the impelling powers muſt be reduced according to the proportional velocities of the mover and moved; or, in levers of different lengths, they may be compared by a ſtandard length of lever, which is the method taken in the ſubſequent reaſoning upon the preceding experiments. An impelling power, therefore, conſiſting of a double weight, or requiring a double weight to counter-balance it, when acting with equal levers, is a double impelling power, or an impelling power of double the intenſity.

OBSER-

OBSERVATIONS AND DEDUCTIONS FROM THE PRECEDING EXPERIMENTS.

1ft, By the firft experiment it appears, that the mechanic power employed, confifting of 8 ounces in the fcale, deliberately defcending (by 5 turns of the bigger barrel) through a perpendicular fpace $25\frac{1}{4}$ inches, will reprefent the quantity of mechanic power which caufes the two heavy bodies, from a ftate of reft, to acquire a velocity, fuch as to carry them equably through 20 circumferences of their circle of revolution in the fpace of 29″; and that the time in which the mechanic power produced this effect was $14''\frac{1}{4}$, as appears by column 6th. And this mechanic power we fhall exprefs by the number 202, the product of the number of ounces in the fcale multiplied by the inches in its perpendicular defcent, for $8 \times 25\frac{1}{4} = 202$.

2d, By the fecond experiment, as 10 turns of the fmaller barrel are equal to the fame perpendicular height as 5 turns of the bigger, it follows, that the fame mechanic power, *viz.* 202, acting upon the fame heavy bodies to accelerate them, produces the very fame effect in generating motion in the bodies as it did before, *viz.* 20 revolutions in $29''\frac{1}{4}$, the fmall difference of $\frac{1}{4}$ of a fecond being no more than may reafonably be attributed to the unavoidable errors arifing from friction of the machine, want of perfect accuracy in its meafures, refiftance of the air, and imperfections in the obfervations themfelves, which muft not only be allowed for in this, but

the reſt; but as the impelling power is acting here upon a lever of but half the length, and, conſequently, but half the intenſity, when referred to the bodies to be moved, it takes juſt double the time to generate the ſame velocity therein.

DEDUCTION. It appears from hence, that the ſame mechanic power is capable of producing the ſame velocity in a given body, whether it is applied ſo as to produce it in a greater or a leſſer time; but that the time taken to produce a given velocity, by an uniformly continued action, is in a ſimple inverſe proportion of the intenſity of the impulſive power.

3dly, The third experiment being made with $2\frac{1}{2}$ turns of the leſſer barrel, the ſame weight in the ſcale of 8 ounces deſcending only one quarter part of the former perpendicular, the mechanic power employed will be only one quarter part of the former, *viz.* $50\frac{1}{2}$; but as one quarter part of the mechanic power produces half of the former velocity in the heavy bodies; that is, they make 20 revolutions in $58''\frac{1}{2}$; that is, nearly 10 revolutions in $29''$; we may conclude, in this inſtance, that the mechanic power, employed in producing motion, is as the ſquare of the velocity produced in the ſame body; and that the velocity produced is as the time that an impelling power, of the ſame intenſity, continues to act upon it, as appears by the near agreement of numbers 2 and 3, column 6th.

4thly, In the fourth experiment, the apparatus is the ſame as the firſt, only here the weight in the ſcale is 32 ounces; that is, the impelling power is the quadruple of the

the firſt, and hereby a double velocity is given to the bodies; for they make 20 revolutions in 14″, which is a ſmall matter leſs than half the time taken up in making 20 revolutions in the firſt experiment. It alſo appears, that the velocity acquired is ſimply as the impelling power compounded with the time of its action; for a quadruple impulſion acting for 7″ inſtead of 14″ generates a double velocity, while the mechanic power employed to generate it is quadruple, for $32 \times 25\frac{1}{4} = 808$. And here the mechanic power employed being four times greater than the firſt, it holds here alſo, that the mechanic power, to be neceſſarily employed, is as the ſquare of the velocity to be generated; that is, in the ſame proportion as turned out in the third experiment, where the mechanic power employed was only a quarter part of the firſt.

5thly, The fifth and ſixth experiments were made with a mechanic power four times greater than thoſe employed in numbers 2 and 3 reſpectively; and ſince the ſame deductions reſult from hence as from numbers 2 and 3, they are additional confirmations of the concluſions drawn from them and from the laſt article.

6thly, In the ſeventh experiment, the diſpoſition of the apparatus is the ſame as number 1, only here the bodies are placed upon the arms at the half-length; from whence it appears, that the ſame mechanic power ſtill produces the ſame velocity in the ſame bodies; for though 20 revolutions were performed in $14\frac{3}{4}$″ (ſee column 7)

 which

which is nearly half the time that 20 revolutions were performed in the firft experiment; yet, fince the circles in which the bodies revolved in the feventh are only of half the circumference as thofe of number 1, it is obvious, that the abfolute velocity acquired by the moving bodies in the two cafes is equal. But, by column 6th, the time in which it was generated is only half; yet, notwithftanding, this will coincide with the former conclufions, if the intenfity of the impelling power is compounded therewith; for, though the barrel was the fame with the fame number of turns as in number 1, and therefore the lever the fame, by which the impelling power acted, yet, as the bodies, upon which this lever was to act, were placed upon a lever of only half the length from the center, the impelling power, acting by the firft lever, would act upon the fecond with double the intenfity, according to the known laws of mechanics; that is, it would require a double weight oppofing the bodies, to prevent their moving, in order to balance it. An impulfive power, therefore, of double the intenfity, acting for half the time, produces the fame effect in generating motion, as an impulfive power, of half the intenfity, acting for the whole time.

7thly, The eighth and ninth experiments afford the fame deductions and confirmations relative to the feventh experiment, that the fifth and fixth do refpecting the fourth, and that the fecond and third do refpecting the firft; and from the near agreement of the whole, when the

the neceſſary allowances before mentioned are made, together with ſome ſmall inequality ariſing from the mechanical power loſt by the difference of the motion given by gravity to the weight in the ſcale: I ſay, from theſe agreements, under the very different mechanical powers applied, which were varied in the proportion of 1 to 16, we may ſafely conclude, that this is the univerſal law of nature, reſpecting the capacities of bodies in motion to produce mechanical effects, and the quantity of mechanic power neceſſary to be employed to produce or generate different velocities (the bodies being ſuppoſed equal in their quantity of matter); that the mechanic powers to be expended are as the ſquares of the velocities to be generated, and *vice verſâ*; and that the ſimple velocities generated are as the impelling power compounded with, or multiplied by, the time of its action, and *vice verſâ*.

We ſhall, perhaps, form a ſtill clearer conception of the relation between velocities produced, and the quantities of mechanic power required to produce them; together with the collateral circumſtances attending, by which theſe propoſitions, ſeemingly two, are reconciled and united, by ſtating the following popular elucidation, which, indeed, was the original idea that occurred to me on conſidering this ſubject; to put which to an experimental proof gave birth to the foregoing apparatus and experiments.

Suppoſe then a large iron ball of 10 feet diameter, turned truly ſpherical, and ſet upon an extended plane of the ſame metal, and truly level. Now, if a man begins to

puſh at it, he will find it very reſiſting to motion at firſt; but, by continuing the impulſe, he will gradually get it into motion, and having nothing to reſiſt it but the air, he will, by continuing his efforts, at length get it to roll almoſt as faſt as he can run. Suppoſe now, in the firſt minute he gets it rolled through a ſpace of one yard; by this motion, proceeding from reſt (ſimilar to what happens to falling bodies) it would continue to roll forward at the rate of two yards *per* minute, without further help; but ſuppoſing him to continue his endeavours, at the end of another minute he will have given it a velocity capable of carrying it through a ſpace of two yards more, in addition to the former, that is, at the rate of four yards *per* minute; and at the end of the third minute, he has again added an equal increaſe of velocity, and made it proceed at the rate of ſix yards *per* minute; and ſo on, increaſing its velocity at the rate of two yards in every minute. The man, therefore, in the ſpace of every minute exerts an equal impulſe upon the ball, and generates an equal increaſe of movement correſpondent to the definition of Sir ISAAC NEWTON. But let us ſee what happens beſides: the man, in the firſt minute, has moved but one yard from where he ſet out; but he muſt in the ſecond minute move two yards more, in order to keep up with the ball; and as he exerted an impulſe upon it, ſo as at the end of ſecond minute to have given it an additional velocity of the two yards, he muſt alſo in this time have gradually changed its velocity from the rate of two yards *per* minute to that of four, and the ſpace, that he will of conſe-

quence have actually been obliged to go through in the ſecond minute, will be according to the mean of the extremes of velocity at the beginning and end thereof, that is, three yards in the ſecond minute; ſo that being one yard from his original place at the beginning of the ſecond minute, at the end of it he will have moved the ſum of the journies of the firſt and ſecond minute, that is, in the whole four yards from his original place. As he has now generated a velocity in the ball of four yards *per* minute, in the third minute he muſt travel four yards to keep up with the ball, and one more in generating the equal increment of velocity; ſo that in the third minute, he muſt travel five yards to keep up the ſame impelling power upon the ball that he did in the firſt minute in travelling one, ſo that theſe five yards in the third minute, added to the four yards that he had travelled in the two preceding minutes, ſets him at the end of the third minute nine yards from whence he ſet out, having then given the ball a velocity capable of carrying it uniformly forward at the rate of ſix yards *per* minute, as before ſtated. We may now leave the further purſuit of theſe proportions, and ſee how the account ſtands. He generated a velocity of two yards *per* minute in the firſt minute, the ſquare of which is four, when he had moved but one yard from his place; and he had generated a velocity of ſix yards *per* minute, the ſquare of which is thirty-ſix, at the end of the third minute, when he had travelled nine yards from his place. Now, ſince the ſquare of the velocity, generated at the end of the firſt minute, is to that

of the velocity generated at the end of the third minute, as 4 : 36, that is, as 1 : 9; and ſince the ſpaces, moved through by the man to communicate theſe velocities, are alſo as 1 : 9, it follows, that the ſpaces through which the man muſt travel, in order to generate theſe velocities reſpectively (preſerving the impelling power perfectly equal), muſt be as the ſquares of the velocities that are communicated to the ball; for, if the man was to be brought back again to his original place by a mechanical power, equally exerted upon the man equally reſiſting, this would be the meaſure of what the man has done in order to give motion to the ball. It therefore directly follows, conformably to what has been deduced from the experiments, that the mechanic power that muſt of neceſſity be employed in giving different degrees of velocity to the ſame body, muſt be as the ſquare of that velocity; and if the converſe of this propoſition did not hold, *viz.* that if a body in motion, in being ſtopped, would not produce a mechanical effect equal or proportional to the ſquare of its velocity, or to the mechanical power employed in producing it, the effect would not correſpond with its producing cauſe.

Thus the conſequences of generating motion upon a level plane exactly correſpond with the generating of motion by gravity; *viz.* that though in two ſeconds of time the equal impulſive power of gravity gives twice the velocity to a body that it does in one ſecond, yet this collateral circumſtance attends it, that at the end of the double time, in conſequence of the velocity acquired in

the firft half, the body has fallen from where it fet forward through four times the perpendicular; and, therefore, though the velocity is only doubled, yet four times the mechanical power has been confumed in producing it, as four times the mechanical power muft be expended in bringing up the fallen body to its firft place.

This then appears to be the foundation, not only of the difputes that have arifen, but of the miftakes that have been made, in the application of the different definitions of quantity of motion; that while thofe, that have adhered to the definition of Sir ISAAC NEWTON, have complained of their adverfaries, in not confidering the time in which effects are produced, they themfelves have not always taken into the account the fpace that the impelling power is obliged to travel through, in producing the different degrees of velocity. It feems, therefore, that, without taking in the collateral circumftances both of time and fpace, the terms, quantity of motion, *momentum*, and force of bodies in motion, are abfolutely indefinite; and that they cannot be fo eafily, diftinctly, and fundamentally compared, as by having recourfe to the common meafure, *viz.* mechanic power.

From the whole of what has been inveftigated, it therefore appears, that time, properly fpeaking, has nothing to do with the production of mechanical effects, otherwife than as, by equally flowing, it becomes a common meafure; fo that, whatever mechanical effect is found to be produced in a given time, the uniform continuance of the action of the fame mechanical power will, in a double

time,

time, produce two ſuch effects, or twice that effect. A mechanical power, therefore, properly ſpeaking, is meaſured by the whole of its mechanical effect produced, whether that effect is produced in a greater or a leſſer time; thus, having treaſured up 1000 tuns of water, which I can let out upon the over-ſhot wheel of a mill, and deſcending through a perpendicular of 20 feet, this power applied to proper mechanic inſtruments, will produce a certain effect, that is, it will grind a certain quantity of corn; and that, at a certain rate of expending it, it will grind this corn in an hour. But ſuppoſe the mill equally adapted to produce a proportionable effect, by the application of a greater impulſive power as with a leſs, then, if I let out the water twice as faſt upon the wheel, it will grind the corn twice as faſt, and both the water will be expended and the corn ground in half an hour. Here the ſame mechanical effect is produced; *viz.* the grinding a given quantity of corn, by the ſame mechanical power, *viz.* 1000 tuns of water deſcending through a given perpendicular of 20 feet, and yet this effect is in one caſe produced in half the time of the other. What time, therefore, has to do in the buſineſs is this: let the rate of doing the buſineſs, or producing the effect, be what it will, if this rate is uniform, when I have found by experiment what is done in a given time, then, proceeding at the ſame rate, twice the effect will be produced in twice the time, on ſuppoſition that I have a ſupply of mechanic power to go on with. Thus 1000 tuns of water, deſcending through 20 feet of perpendicular,

dicular, being, as has been ſhewn, a given mechanic power, let me expend it at what rate I will, if when this is expended, I muſt wait another hour before it be renewed, by the natural flow of a river, or otherwiſe, I can then only expend twelve ſuch quantities of power in 24 hours; but if, while I am expending 1000 tuns in one hour, the ſtream renews me the ſame quantity, then I can expend 24 ſuch quantities of power in 24 hours; that is, I can go on continually at that rate, and the product or effect will be in proportion to time, which is the common meaſure; but the quantity of mechanic power ariſing from the flow of the two rivers, compared by taking an equal portion of time, is double in the one to the other, though each has a mill, that, when going, will grind an equal quantity of corn in an hour.